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ROBERT B. TOLLES AND THE ANGULAR APERTURE QUESTION.

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During the past year the world of science has met with a great, one is tempted to say an irreparable, loss in the death of Robert B. Tolles. It is but a little while since we had to mourn the death of Charles A. Spencer, the pioneer in the work of producing the modern wide angled lenses for the microscope, and now his pupil in the art, who for many years before his death was "easily chief" in it, has followed his old master.

We are meeting but a little way west of the town of Canastota in this rich central region of New York, where these two men had, for a long time, their modest workshop, and from which, whilst it was yet no figure of speech to say they were working in the backwoods, they sent out lenses which no others then made in the world could compete with. The tests which these lenses resolved were for years the puzzle and the despair of the microscopists of Europe, and the glasses would be ranked in the first class even in these days, when we think we are approaching the limit of what the optician can do in aid of scientific research.

I do not mean to enter on the task, grateful as it would be, of telling the story of Tolles' life and labors, a story of a long struggle

of invincible will and noble purpose against hopeless bodily disease. That will be presented to you by one of our fellows best qualified to do it by personal acquaintanceship and long familiarity with Tolles' best work. My own purpose is to give a historical sketch of the great debate over the practicability of enlarging the aperture of microscope objectives beyond the maximum angle in air, in which Tolles' part was, by common consent, the leading one practically. I think a review of the discussion will show that he deserves to rank high in it, also, for clear and accurate scientific comprehension of the principles to be applied; and that he had passed beyond the field of the skillful artisan, into that of a systematic and able investigator, who worked toward valuable results by the proper application of well understood laws.

I think it desirable, also, that the records of our Society should contain a connected account of a controversy which resulted in the triumph of very important improvements in the microscope, which had their practical origin and their first conclusive theoretic demonstration in this country. When I began the preparation of this address, I did not anticipate that we should have the pleasure of welcoming to this meeting the President of the Royal Microscopical Society of London, Rev. W. H. Dallinger, and his colleague of the Visiting Committee from that famous Society; but I regard myself fortunate in speaking before one who was not only cognizant of each step in the discussion which I have to trace, but who may justly claim to have been "*magna pars*" in it, since he was one of the first to apply the improved glasses to delicate and important investigations in biology which could not have been made without them, and has justly earned wide renown both from the accepted results of his work and from the admirable methods and skill with which the investigations were conducted.

It was a noteworthy fact in the great debate upon "Aperture," that it opened with a sharp, though brief, presentation of the opposing views, after which there was silence for many years, during which the American opticians, Spencer and Tolles, were perfecting their work, and the latter preparing to make an objective which was to become more famous, perhaps, than any other bit of glass ever was. I trust that it reposes safely in the cabinet of the Royal

Microscopical Society, where it may for centuries be pointed out as the scientific instrument over which a great historic battle was waged.

In the *Quarterly Journal of Microscopical Science* for July, 1854, p. 214, Mr. F. H. Wenham in an article on Aperture of Object-glasses in which he was discussing the extent to which the aperture of objectives was, as he said, "reduced" when used on objects mounted in *balsam*, from that which the same objectives had when used on objects mounted *dry*, referred to a statement of Prof. J. W. Bailey of West Point, made two years before, in which, speaking of a new glass by Spencer of very large aperture ($172\frac{1}{2}°$) and of its performance on the most difficult tests known, Prof. Bailey had said, "In all these cases I mean the *balsam-mounted* specimens. The *dry* shells I never use as tests." Of this Mr. Wenham said, "This assertion seems to me to be extraordinary, and very like saying that an aperture of 85° or 90° will do everything that is required. I have invariably found that when very difficult tests are mounted in balsam, I cannot discover the markings, and certainly the reasons herein given will account for it. It is to be hoped that the American opticians have discovered some new and peculiar principle in object-glasses, that will render a smaller amount of aperture serviceable, but however this may be, I think that Prof. Bailey's statement requires some further explanation."

To make Mr. Wenham's position plainer, it may be well to say that in the preceding part of the same article, he had described a method of measuring the aperture of object-glasses which had been given in a paper read before the Royal Irish Academy, in January of the same year, by Prof. T. R. Robinson, then or afterwards its President. Of it Mr. Wenham says that it is perfectly accurate in principle, and quotes Prof. Robinson's description of the method as follows, viz: "As a lucid point in the focus of an objective sends out from the eye-piece rays nearly parallel, so light sent in the opposite direction through the microscope will converge at that focus, and then diverge in a cone, whose angle equals the aperture of the objective. If this cone be intercepted at right angles to its axis by a screen, and the diameter of its section, together with the distance of the screen from the surface (focus) of the objective, be carefully measured, they give the aperture." (*Id.*, p. 212.)

Mr. Wenham made a modification of Prof. Robinson's apertometer by using a piece of glass half an inch thick, of which the back was lightly smeared with wax. He focused the microscope upon the front of the glass. Then, throwing light from a candle down the tube, the waxed surface of the glass acts as a screen, and he measured the angle in glass (and approximately in balsam) as Prof. Robinson had measured it in air.

It is interesting to note, in passing, that the device used by Mr. Wenham is essentially the same as one which Mr. Tolles used much later in the controversy, and which Mr. Wenham then repudiated. It has still later become even better known in the application of it by Prof. Abbe to his apertometer.

Applying this instrument to his objectives, Mr. Wenham found that his one-twelfth objective of 146° in air "was reduced to 75° on an object in *balsam*; a one-eighth of 125° to 71°; a one-fifth of 105° to 68°; and a four-tenths of 90° to 56°." He also (still agreeing with Prof. Robinson) stated clearly the proposition that aperture is just effective in proportion to the quantity of *radiations* collected from the object, which, for the purpose of the investigation, may be considered self-luminous. With equal clearness he declared that the separation of distances and definition of tests is entirely dependent upon aperture, and not upon illumination, as the latter will be quite ineffectual without the former. (*Id.*, pp. 216, 217.)

Summing up the results of Prof. Robinson's and his own work, Mr. Wenham reiterates an opinion he had expressed at an earlier date, that 150° (measured in air) "might be considered as the limits of useful aperture," but "now that 160° and even 170° are not uncommon, I consider it quite absurd to suppose any wonderful effects will be produced from an extra 2½°."

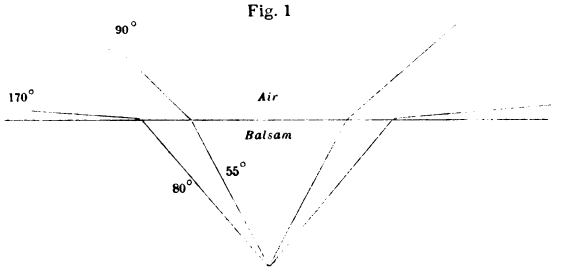
The half contemptuous reference to the "new and peculiar principle" which he ironically hoped the American opticians had discovered, and to the "extra 2½°" in the Spencer objective which Bailey had used, were to become memorable. Nearly everything Mr. Wenham had contended for in his article was based upon sound principle. He had made the assumption, however, that for the purpose of microscopical vision, a given cone of radiant light from a self-luminous object in balsam is no more effective "in the separa-

tion of distances and definition of tests" than a similar cone in air. This mistake vitiated his reasoning, and was a mere assumption, though a very natural one. He soon became half conscious that it needed a careful examination, but he did not make the investigation, and for twenty years more it was an element of error in his reasoning upon the aperture of objectives.

Prof. Bailey was a naturalist of whom we are rightly proud. His study of the diatoms had greatly increased our knowledge of these fascinating forms of life, and had made him seek the best glasses for his microscopical work. He had thus "discovered" Spencer, and had taken pains to let the world know the superiority of his lenses. He replied (through *Silliman's Journal*) to Mr. Wenham's criticisms, asserting that not only did he himself resolve the most difficult tests then known, when mounted in balsam, but it "was a matter of every-day amusement with microscopists in this country." He intimated that Mr. Wenham did injustice to the instruments of England and the microscopists there, especially mentioning a glass by Powell which he himself had published the performance of in 1853. He declined to enter into the defence of "large angles of aperture," contenting himself by saying that Mr. Wenham's error of argument would be "sufficiently obvious to any one who will trace the course of a divergent pencil *out of* the balsam, instead of *into* it."

Prof. Bailey's meaning is, that treating the object under the microscope as self-luminous (as Mr. Wenham had also done), and tracing the cone of rays outward, the angle of 80° in *balsam* became 170°

Fig. 1



in *air*, according to Mr. Wenham's own statement and diagram (fig. 1), just as the angle of 55° in *balsam* became, in like manner, 90° in *air*: that the whole of the aperture of the glass of 170° was used in

getting the 80° in balsam, and that therefore Mr. Wenham was wrong in saying, as he had done, that if Prof. Bailey really resolved the difficult tests in *balsam*, it proved that there was no need of any glass with aperture in excess of "85° or 90°." The increase of the aperture in air represented a proportional increase of aperture in balsam, and the more of either that could be got the better. Instead, therefore, of stopping at 150°, which Mr. Wenham had suggested as the "limit of useful aperture," Bailey advocated the largest angle which could be made, if well made, and said confidently that "no one who has once employed a properly corrected object-glass of large aperture will ever be satisfied with one of a different construction."

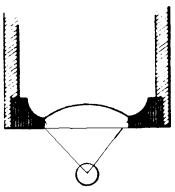
This article was republished in the London *Quarterly Journal*, Vol. iii., 303, and Mr. Wenham rejoined, insisting that to dispute the assertion that "balsam does diminish the angle of aperture of the microscopic object-glass when in action for viewing a structure" implied either ignorance of the first laws of incidence and refraction, or a doubt of their accuracy, and he would not argue either of these points. This was so evident a shifting of the question that it is hard to believe that Mr. Wenham did not see that his former position was untenable. Prof. Bailey had assumed as much as Mr. Wenham that the 146° in air had become 75° in the balsam. The simple question was, did this prove that if tests were resolved in balsam no greater angular aperture would be needed to resolve them in *air* than was thus used in *balsam*; or, in other words, must Prof. Bailey have been mistaken in saying he had resolved tests in *balsam* with a dry glass of 172½° which a dry glass of 85° or 90° would not resolve in *air?* It was this assertion which Mr. Wenham had said in his first article was "extraordinary, and very like saying that an aperture of 85° or 90° will do everything that is required." It is well also to remember that immersion glasses had not yet come into use, and that at this stage the discussion on both sides had reference to dry glasses.

Mr. Wenham then passes (in this second article) to some very interesting experiments in placing a minute object in the center of a spherule of balsam, for the purpose, as he said, of utilizing the "full aperture of the object-glass." His idea was, as he had before

explained it, that the reflections and refractions from the surfaces of the cover-glass, in dry mounting, interfered with the best action of the glass. If the rays of light radiating from the illumined object should emerge from the mounting as radii of a sphere, and the object were in the focus of the objective, these rays would pass into the lens without any refraction or reflection. To this he attributed the better resolution which he admitted might be thus gained (fig. 2).

Meanwhile the subject attracted the attention of others. Dr. F. d'Alquen showed by interesting experiments that the wider angled glass maintains its relative superiority when the object is illumined with a very small beam of central light (*Q. M. J.*, iii., 47). In the same volume (p. 86) Mr. Sollitt of Hull gives his experience, saying that he had never been able to resolve the striae on *Navicula rhomboides* when mounted *dry* with any glass of less than 120°. But, as Mr. Wenham had shown that no glass then made had over 80° angle in balsam (and a dry glass of 180° would have only 82° in the denser medium), it would seem to follow that no glass whatever could resolve the *N. rhomboides* in *balsam*. Yet he had done it better in balsam with a glass of 150° than he had ever.succeeded in doing it in a dry mount ! He asks Mr. Wenham how this can be. It is in answer to this "home-thrust" that Mr. Wenham gives the attempted explanation which I have already referred to, but he adds that this question of resolution "requires an investigation that I have not yet had time to devote to it." (*Id.*, p. 162.)

Fig. 2



In his article in reply to Prof. Bailey, Mr. Wenham gives another experiment, in which he cemented a small lens, nearly a hemisphere, upon the cover-glass of a balsam-mounted object, so that the object, *Pleurosigma formosum*, mounted on the cover should be in the center of the hemisphere. He says the markings "are remarkably well displayed" (*id.*, p. 303) ; but he seems entirely to have overlooked the fact that the lens thus used necessarily entered into combination with the other lenses of his objective, and made a fourth system, changing the power of his objective and making in fact a new, homogeneous immersion lens of it. So near was he to

improvements afterward made, and of which he, by a strange fatality, was destined to be the leading opponent!

Early in 1856, Mr. Wenham (*Q. M. J.*, iv., p. 86) takes pains to emphasize, with excellent spirit, the fact that he had recalled his remarks implying doubt of Prof. Bailey's having really resolved the difficult balsam tests which had been the occasion of the first controversy. He then repeats his argument concerning the "reduction" of the aperture of an object-glass when used on balsam mounts, adding the evidence found in the total reflection of light incident upon the surface of glass at the critical angle of 41°, as a demonstration that the angle within the glass, or balsam, made by rays proceeding from a luminous object can never be in excess of 82°, as all beyond that will be reflected and will not emerge from the cover-glass into air. This he regards as a sufficient answer to what Prof. Bailey had said relative to tracing the angle *after* the rays had emerged.

To this in the same volume (p. 161) Prof. Bailey replies, saying that he has not been guilty of such nonsense as asserting that there is no total reflection of rays of light within glass or balsam; but the pencil of 82° which does emerge, forthwith expands by refraction to 180° in air, and the wider the angle of your objective may be, up to the 180°, the more of these rays it will collect, and the greater resolving power it will have. This, as he very rightly claims, confirms his statement that large angles of aperture are as useful for balsam-mounted objects as for others.

With a friendly note of regret on Mr. Wenham's part that any misunderstanding had arisen, this introductory chapter of the controversy closed. He does not seem to have been satisfied, himself, as to the reasons why the smaller angle in balsam should be equivalent for optical purposes to the much larger one in air, and here the matter rested for some fourteen years. It is worth noting as we pass, however, that in this same year, 1856, Mr. Wenham introduced the small right-angled prism attached to the under side of the slide by balsam, as a means of giving black back-ground illumination, and devised a flat-topped paraboloid as an immersion illuminator for the same purpose. He also inserted a small lens in the top of his concave paraboloid, to be used with the same effect. In the same

volume of the *Quarterly Journal of Microscopy* in which these devices are described, Dr. Charles Hall, of Sheffield, describes the use of the ordinary spot-lens to give bright oblique illumination with objectives of sufficient aperture, in the same way in which Mr. Samuel Wells, of Boston, some years later, used the Wenham oblique illuminator for that purpose. These devices show a progressive knowledge of the conditions of the use of oblique illumination with the compound objective, and are part of the history of the improvement of the instrument closely connected with the increase of its aperture.

In 1870 a singular discussion arose regarding the true marking of the Podura scale, in which a series of articles by Dr. Royston-Pigott in the *Monthly Microscopical Journal of London* attracted much attention. About the same time Mr. Charles Stodder was claiming to have resolved the nineteenth band of Nobert's test-plate with a Tolles immersion objective, a one-fifth of about 175° aperture. Immersion glasses had by this time come into general use, and Dr. Pigott's articles included a discussion of their qualities and advantages. Mr. Wenham entered with his usual force into the debate, and, in the June number of the *Journal*, made the noteworthy suggestion that, if a medium (immersion) of similar refraction to the glass were to be used, no adjustment would be required for any thickness of cover. The principle of the homogeneous immersion glasses was here distinctly stated, and justice demands that it should not be forgotten. The inexplicable marvel is that he should not have seen the relation of this principle to the general question of aperture which was now to be brought again into debate, opening anew the famous "battle of the glasses." In the January number of the *Journal* (1871) he attributed the excellence of immersion glasses chiefly to the perfect correction that the adjustable thickness of the water stratum affords in compensating for every thickness of cover. He incidentally argued that the immersion principle did not give increase of aperture to an objective, sticking to the idea that the angle at which rays proceed from an object constitutes its aperture, and therefore, if the front of the objective is immersed in a homogeneous medium, a glass constructed as a dry glass for the maximum angle in air could not have an aperture greater than 82°

in the balsam. He does not quite return to his old notion that this 82° in balsam is no more efficient than the same aperture in air, but he gives no intimation of having solved the problem of its greater resolving and defining power.

It must be admitted that his opponents had not solved the problem in theory, either, though they held tenaciously to the practical fact that an angle of 82° in balsam was somehow *equivalent*, for optical purposes, to 180° in air. Dr. Pigott, however, without apparently catching the force of Mr. Wenham's argument, attempted to show in reply (same volume, page 70) that it was possible for rays passing from water into the front lens of the objective to make an interior angle of 124°, instead of the 82° to which rays passing from air were limited. Mr. Wenham seems to have seen and to have admitted that rays of any angle might enter the front of the objective, but he was apparently dominated by a fixed idea that no combination of back and middle lenses could be made, which would take up and bring to an aplanatic focus behind the objective, rays in excess of the angle of 82° in the front.

His position, that a glass made to work at its greatest aperture *dry* would not have its aperture increased by the mere substitution of a denser immersion medium for air, was correctly taken; but he made the fatal error of assuming that no glass could be made with back and middle combination which would take up a greater pencil of rays than the maximum for dry glasses. In the March number of the *Journal* (p. 118) he said, "I challenge Dr. Pigott, or anyone, to get through the object-glass into the immersion front, a greater angle, or any portion of the extraneous rays that would in the other case be totally reflected, *as no object-glass can collect image-forming rays beyond this limit.*"

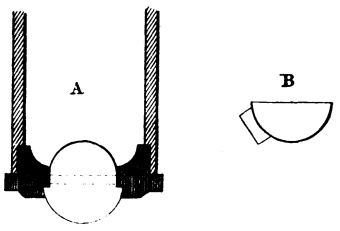
Let us fairly recognize the fact that "the object-glass" to which he referred, was the one of which he had given a diagram, or one of similar construction, of an aperture less than 180° in air. Of such a glass his proposition was true. His fatal error, and the source of all his woes in the succeeding debate, was the false generalization which I have italicised.

It was this challenge to "anyone," and this limitation of the capa-

bility of *any* object-glass, which brought Mr. Tolles into the arena of the debate. We shall see how he bore himself in it.

In the July number of the *Journal* (1871, p. 36), without any reference by name to any disputant, and in the most absolutely impersonal manner, he begins a little article with "The following is given to illustrate the comparative available angle of dry and immersion objectives." Then follows a brief description of what has since become well known, as an immersion illuminator, under the name of his "traverse lens." In this case he applied the little hemispherical lens directly to the front of an objective which had an angle of over 170° (fig. 3, A). A film of air being between the traverse lens and the objective front, the interior angle in the latter was, of course, less than 80°; but upon flowing a drop of water between the front and the illuminating lens, the interior angle was found to be over 100°. The little facet (B), applied to the illuminating lens, enabled him to convert it into a prism with a face at right angles to the immergent ray, which was not refracted but proceeded by a radial line to the object. The same device enabled him to limit the illumination to this small beam of light, and to measure its angle with the axis of the objective. Blood discs in the immersion film thus introduced were distinctly seen under this illumination, demonstrating that the rays were image-forming rays.

Fig. 3



No demonstration could be neater or more conclusive, nor could it more perfectly cover the whole ground. Mr. Tolles said it had already been in use "a year or two here," meaning in Boston, and with his own wide-angled immersion lenses. It meant that immersion lenses of greater aperture than 82° in balsam (or the equivalent 180° in air) had already been made and were in use, and that the appropriate method of illumination for the full use of the aperture had been perfected also, accompanied by a practical means of demonstrating the angle.

Coleridge, in his *Biographia Literaria* lays down the good maxim, "Until you understand a writer's ignorance, presume yourself igno-

rant of his understanding." In obedience to it, I have tried to discover how it happened that a man of Mr. Wenham's familiarity, both practical and theoretical, with the ordinary laws of optics should have become committed to a palpably false position involving their contradiction. In all sophistical reasoning by which acute and able minds deceive themselves, there is usually some omission of an important condition, or some assumption of a premiss as established, which puts the mind in a false position, and the attention being riveted upon the intermediate steps of the argument, there is a temporary blindness to the real point of difficulty.

In looking back at the controversy it seems to me that the modesty of Mr. Tolles' way of stating the characteristics of his objective, acted upon Mr. Wenham as if it had been a trap laid for him, into which he had fallen headlong. Mr. Tolles does not say, in so many words, that his objective was made with an aperture beyond the maximum in air. He only states a proof of it. Mr. Wenham in his reply (p. 84 of the same volume) assumes that the 170°, or thereabouts, in air, *is the maximum angle of the glass*, and on this assumption we may pardon the contemptuous way in which he performed his easy task of demonstrating that such a glass could not possibly have more than 80° of aperture in balsam, and that Mr. Tolles was, of course, all wrong in supposing that he had measured it differently. It was the old story—your pencil 170° in air *must* be reduced to 80° in balsam, and you contradict the first laws of refraction when you deny it. He even missed, absolutely and totally, the point in the use of the movable facet on the traverse lens, and complacently points out the refraction on the spherical surface which Tolles had by this means provided against and sufficiently explained. He went further, and committed himself again to the erroneous proposition that "80° is the utmost aperture that we can expect to obtain for an object *mounted in balsam*, and the principle does not differ, whether we employ an immersed front or not." He thought he understood Tolles' ignorance, but alas, the fact was he was himself ignorant of Tolles' understanding. In this, as in others of Tolles' own articles, the latter assumed that his readers could readily see the application of the laws of optics, and the brevity which came of this assumption was again and again put

down as unlettered ignorance when it was simply scientific argument a little too compact for them. It is profitable, if humiliating work, to go back and read that first article of Tolles, in the light of present knowledge, and to see how lucidly clear and impregnable a statement it was. Every word is evidence that it was he who was master of the subject.

The mistake which Mr. Wenham made accounts for the manner in which he closed his reply. He repeated the demonstration, by the use of the tank, of the diminution of the air angle when a balsam immersion is used, and then concludes thus—"Messrs. Tolles and Stodder have girded on a convex front, and then ventured forth to make a stand against my unwelcome statements—that the aperture of object-glasses *is* reduced on balsam-mounted objects, and that there is no subsequent increase of this aperture by using an immersion lens. I had not the pleasure of shaking hands with them before the collision, but in the absence of this ceremony I hope that they may take the reception that they have met with, in good part." It would be ungenerous to recall the supercilious tone in which this rejoinder was written, if it had not exerted a positive influence upon the progress of investigation. It was destined to cost Mr. Wenham dear, but this was of little moment compared with the fact that his great influence among English microscopists made his contempt a powerful weapon in such a controversy, and one that he had no right to use unless he was quite sure he understood his opponent's ignorance. His followers echoed his sarcastic laughter, and for a time it looked as if Tolles would be sneered down for a charlatan who was puffing his wares by pretending they had qualities which they demonstrably did not possess.

He was thus responsible for introducing into the discussion an element of personal feeling, which proved an opaque veil, shutting out the "dry light" of science, and which prevented him and his numerous and respectable followers from accepting results of great moment to scientific investigation. Their influence controlled the Royal Microscopical Society for a long period, and through it the great body of British microscopists. It is not going too far to say that the few injudicious words thus used by Mr. Wenham delayed for years the progress of microscopical research throughout a large

part of the world. His authority was deservedly great. His improvements of the microscope and its accessories were so numerous, so beautiful, and so useful, as to excite the enthusiasm of all who used the instrument. He had made himself an expert in the construction of object-glasses, and in every department of his activity, he had, with a noble disinterestedness, made the world a free gift of his inventions. That his influence was great among microscopists was therefore a matter of course, and this makes it all the more to be regretted that this influence should have become an obstruction to progress.

I may be pardoned for delaying a moment here, to speak of the absolute necessity of excluding personal feeling from scientific discussions. It has grown into a proverb that the wisest criticism of the literary performances of college students is to advise them to cut out all the passages they think particularly fine. On a similar principle writers upon scientific subjects may properly be exhorted to strike out of their papers all the "good hits" which they have made at opponents of their opinions. No mixing of the wrong chemicals in your experiment will more surely ruin it than a "little dig" at an opponent will spoil the most promising investigation of a scientific truth. The "personal equation" is troublesome enough at best; but when pride of opinion, the difficulty of retracting a mistaken argument and of summoning the candor necessary to the admission of an error, are all increased by the consciousness of sharp words spoken in public which make retreat more or less humiliating, profitable discussion is out of the question. It becomes only a struggle for personal triumph and not for truth. And the worst of it is that the heat which is engendered prevents the disputants from knowing how far they have departed from the scientific spirit. Here more than anywhere else we may apply the scripture injunction to "leave off contention before it be meddled with."

In the September number of the *Journal* for the same year (1871) Dr. J. J. Woodward announces the resolution of *Amphipleura pellucida* with a one-fifth objective made by Tolles, and sends to the editor photographs of the shell made in this way.

In the November number, Mr. Tolles again (and still in an absolutely impersonal manner) illustrates the almost self-evident fact

that rays in excess of 82° may be sent through the hemispherical front lens of an objective, by a diagram of two hemispheres cemented together with balsam and with an object in the focus of the objective of which one of the hemispheres is supposed to be the front, cautioning the experimenter that balsam must be used above and below the slide, so as to make complete homogeneity, if it is tried by the substitution of an ordinary slide with the illuminating lens below it. He has thus modified the experiment detailed in his former article, to avoid the apparent point of Mr. Wenham's criticism. He afterward tells us that he did not at this time know of Mr. Wenham's similar arrangement of hemispheres in 1854, and was not aware of the force of the *argumentum ad hominem* he was thus making.

To this illustration by Tolles, Mr. Wenham replies in a note to the editor of the *Journal*, published in the December number, in which he evidently has in mind his own experiment to which I have alluded. "If he is clever enough," he says, "to mount a diatom in balsam, between two hemispherical lenses, or enclose it in the center of a tiny spherule of hard balsam or gum, then, of course, it can be illuminated from all angles, and seen by the full aperture of an object-glass (the refraction of the material will not influence the result, as the rays proceed in straight lines through the surface). But, if I understand Mr. Tolles' not very perspicuous summary correctly, he brings this profound illustration forward to show that the angle of aperture is in no way diminished, when the object and front lens are both immersed in fluid."

He has here overlooked the plain fact that in Mr. Tolles' diagram both object and front lens *were* immersed in the homogeneous fluid, for the upper hemisphere was distinctly declared to be the front lens of an objective in both Mr. Tolles' articles. He has further mistaken the point Mr. Tolles was proving, which was not, as he says, "that the angle of aperture is no way diminished," but that it may be *greater than 82°* when the object and front lens are both immersed in fluid. Yet with this double blunder dropping from his pen, he so far forgets himself as to speak of the rather hopeless attempt to convince him (Tolles) by explaining primary laws of refraction, or the very A B C of optics. One can imagine the grim

smile with which Mr. Tolles would receive such instruction in the elements of optics.

But in the same letter Mr. Wenham had asked whether Tolles "really expects us to believe" that he can obtain, "by the back combinations alone, * * 170° or even anything near the least angle of 90° that he has shown?" Mr. Tolles had said nothing of obtaining an aperture of 170° in balsam. He had shown that 100° can be obtained, or 110° by Mr. Stodder's measurement. Further than that, he had only said that with a homogeneous immersion front, rays could *enter the front lens* closely up to 180°.

Now, following coolly and patiently the task of furnishing demonstrations of Mr. Wenham's errors, he adopts a diagram Mr. Wenham had before used, and shows how, by the addition of a new and small hemispherical front, and a change in the thickness of the old front, an immersion glass may be made of which, the back and middle combinations remaining the same, an aperture of over 90° in balsam would be constructed, and one approximating 130°, if the immersion medium of higher refractive index were used; for as he says, "only the refractive ability of the connecting medium is the real practical limit of angle of transmitted pencil," &c. (*M. M. J.*, vii., 118.)

In a letter to the same journal, dated Dec. 29th, 1871, he states that he has actually constructed an objective which worked dry with an aperture of 100° at open point, and had placed the tiny spherule in front of it, with blood discs between the hemispheres, and that the glass showed the full aperture of 100° when the blood was fluid, but was reduced to 82° when, by drying, air entered between the hemispheres. These were examples of what have since become so well known as the "four system" glasses. He then avows his belief that immersion objectives may be made approximating 170° aperture in balsam, adding, "but it should be particularly noted that this is not avowed for water as the medium instead of air above the slide cover, but instead of air a medium approaching closely the refractive power of glass."

In the same letter he speaks of resolving *Amphipleura* by sunlight, with the aid of a one-inch objective of low angle swinging under the stage as a condenser. It is matter of history that he used this con-

denser upon a radial arm turning upon the object as a center, exhibiting, by this means, the angle of one of his glasses to Dr. Woodward, who subsequently testified to the fact.

Mr. Wenham's next communication (*id.*, p. 272), which is also in the form of a letter, is not pleasant reading. He repeats that it is not worth while to argue optics with one who do n't comprehend the first law of refraction, at the very moment that he is denying that the addition of a new hemispherical front to a three-system lens makes another objective of it. He admits that it may give to the old glass of 100° aperture in air the same full aperture in balsam, at the very instant that he denies that a greater aperture in balsam than 82° is possible. He criticises lines in the adapted drawing of his own which do not change the principle—Tolles having used Wenham's own diagram as one sufficiently good for an illustration. He says he will strike his colors if Tolles will furnish a diagram showing the passage of rays up to the "full aperture," and even intimates that if the latter has not knowledge enough of geometry to draw the refracted rays, they shall be drawn for him if he will give the figures of the lenses. Remember that this came from a man who was even phenomenally wrong in his application of the first laws of refraction, and was addressed to one who was giving to him and to the world, lesson after lesson in most valuable applications of those laws, the only trouble being that the contemptuous and arrogant pupil did not learn the lesson set.

But Tolles was never stung into an ill-natured retort. He quietly follows up the proof. Mr. Wenham had explicitly approved the balsam tank method of measuring the aperture in balsam, and Tolles furnished to the *Journal* (vol. viii., p. 106) a table of nine objectives actually measured with the tank, and of which all but one had an aperture in balsam of more than 82°. Part of these lenses had compound fronts, and part single ones. "Every objective recorded in the table readily shows the true test, *A. pellucida*, by uncondensed light of a common petroleum lamp flame."

In July of that year (1872), he sent to the editor of the *Journal* an objective which he had made three years before (in 1869). This glass, he said, with the collar adjustment at 10½, showed by the tank method of measurement, an aperture of 93° to 95° in balsam, and he

invited Mr. Wenham to test the accuracy of this assertion. Dr. Josiah Curtis certified to the accuracy of Mr. Tolles' measurement of a glass having 92° balsam aperture, but it does not clearly appear that it was the glass in question.

In the November number of the *Journal*, Mr. Wenham produced a new diagram to prove what nobody had questioned, that if a glass had a maximum angle of 170° in air, and balsam be flowed in between the front and an object, the aperture in the balsam would be found to be about 80°. But in the whole there is the old assumption, the pure assumption, which by this time has grown amusing, that no system of glasses can be combined which will give an aperture beyond his 170° or 180° degrees in air, or the equivalent in balsam. If he would himself, in good faith, have constructed a diagram in the place of that of Tolles, which he found fault with, and placed a hemisphere upon its front so that the rays thrown down the tube could be traced, he would have seen his error. But the singular delusion that the fourth system which Tolles added did not become a new front of the objective and one of the component elements in the compound lens, stuck to him, and he could not be rid of it. He again refers to Tolles' lucid presentation of the scientific truth, as a "would-be scientific demonstration," and as "a muddle and downright nonsense." In the same article he makes a singular allusion to the original controversy with Prof. Bailey and the Spencer glass of 172½°, showing a strangely lingering irritation after a lapse of some eighteen years! Speaking of the difficulty of measuring angles of aperture by the sector accurately, on account of a "border of diffused light," he says, "when statements are brought forward such as 172° '*and a half*,' I cannot help smiling at its absurdity, as a mere piece of brag." Now I will venture for myself a Yankee *guess* that this 172½ which had so long stuck in his memory, was the innocent result of using a sector or protractor divided only to 5° units, and that the half of the unit was the nearest approximate measurement that could be made! If the averaging of results in all sorts of scientific measurements were to be held to be a claim to the accurate determination by manipulation of the fractions which result, all our scientific proceedings are full of this "absurdity" and "brag." But Mr. Tolles had said nothing about

172½°. He had said his glass had 93° or 95° aperture in balsam, leaving a liberal margin for differences in observation of the angle.

He now measured the objective which Mr. Tolles had sent over to be tested by the tank method, which Mr. Wenham had himself approved. There was a very simple thing to be done. The collar was to be set at 10½, the nose of the objective immersed in the balsam of the tank, and parallel rays of light sent through the back of it into the balsam where the illuminated cone of emergent light could be measured. Every microscopist was presumed to know that the angle of a glass used dry was less than when adjusted for immersion, and that a true immersion glass will only perform dry at all by cutting down its aperture. Yet Mr. Wenham, as afterward appeared, set the collar at the point where he found it to work best dry, upon a dry mount of podura scale, with a thin cover-glass. It does not appear whether he used any other than central light. This is only another way of saying that the systems were as nearly *opened* as they would work upon any covered object, and the glass adjusted at its minimum angle. His practical authority in such matters was such as to impose upon the Fellows of the Society who acted as witnesses of the performance, and they certified that the objective showed a balsam angle of only 79°.

I do not doubt that Mr. Wenham was entirely sincere in his subsequent statement that he had overlooked Mr. Tolles' indication of the collar adjustment, and that, as dry Podura scale covered by a thin glass was a favorite test object for some purposes, he thought he was treating the glass fairly. That part of the subject has been long set at rest and Mr. Tolles measurement has been long confessed to be accurate. We may be permitted, however, to say that a microscopist may well wonder that every exclamation point on the podura scale did not become vocal with amazement at this mode of determining the maximum working angle of a wide-angled glass.

Mr. Wenham then made his test the basis for a new assertion that Mr. Tolles was one who would "not be led by any theory," and therefore, by inaccurate and improper methods of measurement had got a false indication of the aperture of his glass. He also gave another and, if possible, a more distinctly erroneous statement of his

own theory on the subject, namely, that in the front lens of an objective "the angle behind the first surface cannot get beyond 82°, and supposing the other lenses to be of such a form as to bring to a posterior conjugate focus the rays of even such an improbable angle, this cannot be increased, either with water or balsam immersion without destroying that focus and giving a negative result with no image in the eye-piece." (*M. M. J.*, vol. ix., p. 30.)

Mr. Tolles protested against the so-called measurement, as no measurement at all. It was, in fact, a proceeding in which the glass was from the first doomed, as *Benedick* says in "Much Ado About Nothing," to a "predestinate scratched face," not through any ill intent on the part of Mr. Wenham and the committee, but through what we must now call a most astounding ignorance of the principles which control the manipulation of a wide-angled objective. On its return to him, Mr. Tolles showed that the 145° in air was found when his glass was measured at "open point" for uncovered objects, and that the equivalent balsam angle at this point was 77°, or less by 2° than Mr. Wenham had found it. He thus established that it was practically its minimum instead of its maximum aperture which had been measured. He earnestly disclaimed any belief in a wrong purpose, and in the most manly way says he will submit the matter to the same committee, by all means including Mr. Wenham, if they will repeat the examination with the tank and a proper adjustment of the glass. He also sends a drawing of his apertometer, which was a semi-cylinder of crown glass, at the center of which immersion contact by balsam was made with the objective, and shutters moving on the graduated rim marked either the emergent rays, when light was thrown down the microscope tube, or the angle of the illuminating pencil when the instrument was used in the ordinary way (*id.*, p. 213). In the well-known Abbe apertometer, the only modification of this is the beveling of the straight edge so that it will act as a prism, and enable us to use the apertometer lying flat on the stage, and in the addition of the indices for numerical aperture to the graduation of the circular edge.

In the same volume (p. 268) Dr. Woodward gives the result of measurements with the balsam tank of a one-fifth objective made by Tolles, in the dark room of the Army Medical Museum at Washing-

ton, where with heliostat he threw a beam of parallel rays of sunlight down the tube, and measured the emergent cone in the tank where it was beautifully illuminated and sharply defined. He certified its balsam angle to be somewhat over 100° at closed point. This was a "four system" glass, which Dr. Woodward thought was specially adapted for the experiment, but which was proven to be a genuinely useful objective by resolving *Amphipleura pellucida* and *Frustulia saxonica* in balsam.

He also makes a brief, but lucid explanation of the error of supposing that no rays could emerge from an objective immersed in balsam, of a greater angle than 82°; that, by the laws of refraction, being the limit of the transmission of light from glass into air only, and having neither application or meaning when the transmission is made into water or into balsam. Prof. Newcomb, the well-known astronomer, editor of the *Official Nautical Almanac*, and Prof. Keith of the National Observatory, both certified to witnessing the measurement of the Tolles glass by Dr. Woodward and to the accuracy of his report (*Id.*, p. 273.)

Unfortunately Mr. Wenham had written an answer to Mr. Tolles' last before he read Dr. Woodward's article, and had called the apertometer a "wretched adaptation, containing the seeds of a crop of refractive errors," and contemptuously says that Mr. Tolles "cannot get beyond the idea of putting hemispherical or semi-cylindrical things of glass in front of the objective, so that the light may emerge without sensible refraction." (*Id.* p. 284.) He seizes upon the idea that if you focus on the front of the apertometer in air and then let in water or balsam, the focal point is changed. This he thought would account for an error in measurement. He repeats that he "may assure Mr. Tolles that the inflexible laws of light will not bend to meet his wishes." He also repeats the fallacy, for a time popular among his followers, that "*mere light* can be seen through a hemispherical lens up to 180°, far beyond the perception of distinct images."

In cruel juxtaposition with this is printed (*id.*, p., 285) a note from Tolles written *before* Mr. Wenham's, in which he quietly "puts on record" the fact that he used a single pencil of light of 112° in glass by an immersion illuminator with one of his objectives, giving a

symmetrical, undistorted view of *Navicula Amicii* as a test, with good definition and resolution. He is thus hunting his opponents "to their holes," to use a pithy frontier phrase.

But Mr. Wenham had even gone farther. Failing to see the beauty of Mr. Tolles substitute for the inconvenient balsam tank, he not only sneers at it, but wrongly assumes that Tolles used it because the tank had decided against him. "With the tank," he repeats, "nothing can go wrong." We may fairly date the decisive shock to Mr. Wenham's reputation as an authority in practical microscopy from that number of the *Journal*, in which this letter of his was interleaved between Dr. Woodward's report on the measurement with the tank "which could not go wrong," affirmed by Profs. Newcomb and Keith, and Tolles' own record of resolving *N. Amicii* with a balsam angle of 112°.

Mr. Wenham's reply to Dr. Woodward impresses one with the feeling that he was conscious of error, but the open contempt with which he had treated the positions and arguments of Mr. Tolles, made it impossible for him to retract. Tolles had suggested the "semi-cylindric affair" for those who could not conveniently use the balsam tank, and Wenham now suggests a square glass, for "those who still doubt the loss of aperture on objects mounted in transparent media"—the imaginary men of straw whom he is thrice destroying—yes, "*ter, quaterque.*" He seizes upon Woodward's statement that when the tank was used in the common way, there was no sharp definition of the cone of emergent light from the objective at "closed point," but fails to notice how this, in the use of it in the dark room, became "a superb amber-colored triangle of light, * * the sharp, well defined edges of which permitted the angle at the focus to be measured with ease by a card-board protractor held beneath the flat tank," or the fact that Woodward found the lens to resolve and define well at this same "closed point." He then grasps at Woodward's statement that the one-fifth examined by him had been adapted with a fourth lens in front, omitting to notice that Tolles had secured the extra angle in both "ordinary" three-system and special four-system lenses, and claims this as an invention of his own, while asserting that it does not give any increased aperture to the objective! He, who assumes his own

proficiency and Tolles' ignorance in the laws of refraction, says that "the optical effect of the hemisphere as a lens was *nil*," when it was as efficient in the production of the image at the conjugate focus, as any other lens in the compound objective. After Mr. Tolles had read the controversy between Bailey and Wenham in 1855, he had always said the latter, in his controversy with himself, was the engineer "hoist with his own petard," though the engineer had failed to understand the most significant thing in the construction of his instrument. Prof. Keith and others subsequently expressed their amazement that Mr. Wenham so completely failed to see the necessary conclusions from his own experiment.

In the August number of the *Journal*, 1872 (vol. x., p. 58), Mr. Tolles shows that the "cylindrical appliance," with its shutters, was necessary to demonstrate (when used as an immersion illuminator), that the part of the aperture beyond 80° was used. Next, that in all his measurements the object was kept in focus. Finally that the glass apertometer was more true than the tank because if the balsam was *thin*, the apparent angle would be too great, and he had in fact claimed less for his own objective, *under the same conditions*, than Mr. Wenham's tank had given it. He mentions the fact that the "exceptional" one-fifth with four systems, was one of the identical glasses, unaltered, of which he had given a table which had already been published.

The testimony continues to cumulate. Dr. Woodward now measures the identical objective which had been in Mr. Wenham's hands, and reports that by the same tank method which he employed before, and which Mr. Wenham had explicitly approved, it had a balsam angle of 70° at the open point, and 84° when at adjustment for maximum angle; thus conclusively showing that the former measurement was not at the proper angle of the glass. He adds: "Unlike Mr. Wenham, I find this glass performs admirably at the point of maximum angle, provided the cover-glass is thick enough. I tested it on *Grammatophora subtilissima* in balsam under a cover $\frac{1}{75}$ of an inch thick, and obtained what I am obliged to call admirable definition." (*Id.*, vol. x., p. 97.)

In the same volume, p. 210, Dr. Woodward expresses his disappointment that a correct explanation of immersion objectives of

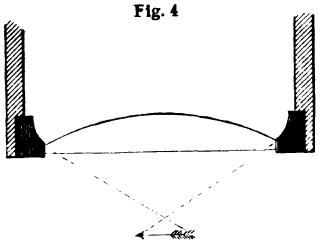
greater aperture than corresponds to the maximum possible for dry ones had not come from Mr. Wenham. He then explains and illustrates the theory of an immersion lens with a medium of the "same refractive power as the crown-glass front." Having done this he answers the question how far the theory has been reduced to practice, by giving the results of the examination of several of Tolles' lenses in the Army Medical Museum. Lastly he exposes the error of saying that the fourth lens, the hemispherical front, is optically "*nil*," by sending photographs of *N. lyra* taken with the "exceptional" one-fifth, with and without the extra front. The one shows 765 diameters magnification, the other 450. Without eye-piece, at 12 inches, the one magnified 60 diameters at open point, the other 39. It was therefore a fifth with the hemisphere, and something between a four-tenths and a one-fourth without it.

It is historically worth noting that Dr. Woodward calls attention (in closing his article) to the fact that in a recent number of Max Schultze's *Archiv für Mikroskopische Anatomie*, Prof. E. Abbe of Jena had asserted the possibility of correcting the spherical aberration for greater angles of aperture than those corresponding to the geometrical maximum for dry objectives.

My purpose being chiefly to follow Mr. Tolles' part in this famous controversy, and my time being necessarily limited, I must omit the succeeding articles in which Woodward and Wenham continue their part of the discussion, as I have already had to omit the part taken by several lesser combatants in the arena. But Mr. Wenham, with a fatal infatuation, could not omit an opportunity for a new outbreak upon Mr. Tolles. Mr. Crisp, the present well-known Secretary of the Royal Microscopical Society, had bought a Tolles glass marked "180°, balsam angle 98°." Mr. Wenham attacks it (*M. M. J.*, xi., p. 112) with a display of vivacious chaff, with capital letters and exclamation points, that would indicate that he had come off at least with satisfaction to himself in the previous encounters. In this article are some extraordinary propositions to come from an expert in optical laws. One would not have expected to find him measuring the working distance of an immersion lens, and then ridiculing the glass because this distance was absurd if you assumed the glass to have an angle of 180° in *the immersion medium*. Yet this was pre-

cisely what he did. He did more also; he assumed—what is false for any lens, single or compound, and what is contradicted by every work on optics from the beginning of the investigation of the properties of lenses,—that the focus of a lens is in the plane of the object, instead of the object being, as every elementary treatise teaches, beyond the focus, or farther from the lens (fig. 4). I have in another place (*Am. M. M. J.*, vol. iii., p. 61) discussed some of the interesting contrasts and relations between working distance and focal length proper, and I will not stop here to say more than that it is by no means true, theoretically or practically, that objectives with the same diameter of front lens, and the same aperture, have necessarily the same working distance. Still less is it true that the angle subtended by lines running from the center of the field of view to the sides of the front lens is the angle of aperture. It is always less than the aperture, often much less.

Fig. 4



This mistaken assumption was, however, made the point of departure for a new series of asserted demonstrations of the charlatanry of wide-angled glasses. In this article an amusing thing occurs. Mr. Wenham admits that the tank method of measuring balsam apertures is "both messy and inconvenient," and after trying a square block of glass which still did not quite answer the purpose, he says (p. 116), "I therefore adopted a semi-cylinder of glass, *such as has been used in the instrument for demonstrating the law of Descartes.* The center of radius was exactly in the polished plane," &c., going on to describe exactly *Tolles' apertometer* and immersion illuminator excepting the very useful movable shutters, the instrument which he had himself greeted with no end of chaff a little before, as a "semi-cylindrical affair" which Tolles so stupidly insisted was of some use in precisely such a measurement.

The measurement actually made by him was a fallacy of incorrect manipulation, as Mr. Mayall proved some time after; but it had its immediate effect at home. He made use, also, of a slit in thin foil at the center of the apertometer, on which the objective was

focused: a proper procedure if properly used. But, as indicating the general use of the apertometer, he now first obtained the full dry aperture on an object mounted in balsam by putting a small hemisphere over the slit and focusing a dry objective upon it; he then flowed an immersion fluid over the hemisphere and up to the front of his objective, and showed that the angle was reduced below 82° in balsam. In short, he did what would be equivalent to filling the space between the front and next system in a modern objective with fluid (water or balsam), and points with triumph to the fact that its aperture is not what it was before! Certainly no one will dispute the truth of such a proposition, but it was none the less an extraordinary one to make in that stage of the discussion.

In a letter published in the same number of the *Journal*, Prof. Keith very briefly stated the reason why the critical angle of 82° had no application to an immersion lens. This is double the angle of total reflection when a ray of light passes out of glass into air; the light being totally reflected at the surface of the glass and not emerging into the air. But when balsam is interposed, so as to surround the radiant object and fill the space to the glass front, there being no difference of refractive power of the two substances, they become one for optical purposes, and it is as if the surface of the glass were abolished. Rays of any angle may then enter the glass from any radiant point, but how great an angle could be made practically available was a question for practical opticians. Tolles had made a balsam angle of 112° available.

After an interchange of several minor articles between Mr. Wenham and others, which had resulted in his declaring a purpose to drop the discussion, though he would still respect a diagram drawn under the conditions named, Mr. Tolles, who had been content to leave the discussion with such contestants as Dr. Woodward and Prof. Keith, comes forward again with a diagram showing a construction of an objective with a balsam angle of 90°, with a single hemispherical front, and a "back-and-middle" combination of 60°. He also shows neatly, by the same diagram, how, when the glass is worked "dry," a part of its aperture is unused. Consequently, when a glass of more than 82° balsam angle is used dry, the angle actually used will be inappreciably less than 180°, and it is no ab-

surdity to call it a dry glass of that angle, so that Mr. Wenham's hilarity on that subject was uncalled for. He then, without one jeering syllable, remarks that, "Mr. Wenham having adopted the semi-cylinder," he will give some hints as to its more convenient use, which he proceeds to do, showing that he himself used it in such a way as to exclude from the illumination of a test object, all light except rays beyond the maximum for a dry glass, and still had sharp definition of the object, and therefore image-forming rays (*M. M. J.*, xii., p. 13). A little later he supplements this by putting an opaque stop on the back lens, which shut out all light when the glass was used dry; then, using it by immersion, threw in light beyond the 82° in glass, and had both resolution and definition on a *N. Rhomboides* in focus all the while. (*Id.*, p. 62.) Nothing neater or more decisive is conceivable. He also calls attention to the error in stating the working distance of the Crisp glass when used dry. There was no appreciable working distance at all when so used, but the object was in contact with the front.

Mr. Tolles now did what, for a poor man dependent upon the sale of his lenses for a livelihood, was no small sacrifice. He gave to Dr. Woodward the formula of one of his objectives, and authorized him to make and publish the computation of all the curves and lines of refraction, so that the fact that the glass had a greater aperture than 82° in balsam might be mathematically demonstrated. The results were published in an article by Prof. Keith in the September number of the *Journal*, 1874, vol. xii., 124, and were accompanied by photographic copies of the detailed computation, for distribution to those who might wish to follow the calculation for themselves. The computation was accompanied by a diagram giving the actual sections of the lenses and the course of the rays traced through them geometrically. Every optician could now copy Tolles' lenses.

Dr. Woodward followed Prof. Keith's article with another of his own in which he said the glass selected was the one-tenth belonging to the Army Medical Museum having "superb definition" as shown by photographs of *Amphipleura pellucida*, and which had a single hemispherical front. Its computed angle was 110½° in balsam, or allowing for the encroachment of the brass-mounting on the front,

87°. But a statement of Dr. Woodward in this connection has such interest in connection with the subject of homogeneous immersion that I must quote it in full. He says Prof. Keith "has computed the sperical aberration of the combination, adjusted as above, and finds it practically *nil.* This being the case, the objective ought to perform well when adjusted to the point of maximum aperture if *balsam* be used as the immersion fluid in lieu of water and the thick cover ordinarily employed at this position of the screw-collar. Accordingly, in company with Mr. Keith, I tested the objective in this way on *Grammatophora subtilissima* by lamplight, and we both thought the definition unmistakably better than with water immersion." (*Id.*, p. 127.) Tolles had therefore not only given to the world admirable homogeneous immersion glasses, working practically and well in an immersion medium of the same index as the glass front, but he had now accompanied them with a complete formula and diagram from which whoever chose might copy them.

Dr. Woodward reminds Mr. Wenham of his promise to examine with care a figure "with the rays carried to their final destination," and his assurance, "if I saw no error, I could not state that there is one, and I trust that I have the candour to admit accuracy." (*Ibid.*) In his rejoinder, however, Mr. Wenham fails to criticise the diagram or candidly to admit its accuracy. The curves of the lenses had been given and the refractive index of every piece of glass, and both the trigonometrical and the geometrical demonstrations had been given, yet the astounding answer is, that because the diagram has been accurately constructed in accordance with the computed results of refraction through the lenses, it has therefore been drawn to suit the proposition and may be dismissed." (*Id.*, p. 221.) In the face of the mathematical demonstration, however, he takes comfort in the result of his measurement by the slit method, and insists that it has given the death-blow to Mr. Tolles' extra apertures. (*Id.*, p. 222.) He concludes, "There I leave him, not without some amusement at the grotesque fatuity that induced his colleague (Mr. Stodder) to select the chaste and Christian-like motto, 'A blunder is worse than a crime.'" (P. 223.)

Still abstaining from any word of discourteous retort, just as he had followed his opponent from the "spherules" to the tank with

which "nothing could go wrong," from the tank to the glass apertometer, and from the apertometer to the diagram and computation of the formula of his homogeneous immersion one-tenth, Tolles now accepted the slit method of measurement, and constructed it by cutting a line with a knife edge through silver amalgam on a slide. In this slit he mounted test diatoms so that they were in balsam in half the slit and dry in the other half. By this ingenious arrangement he was able by merely moving the slide to measure the angle of the objective either upon the balsam mount or the dry mount, the test diatom being in focus and resolved all the time. He did not need a slit as wide as the field of view (as Mr. Wenham had conceded), but could use one much narrower, thus surpassing the stringency cf his adversary's conditions. He quietly notes the fact that the measurements when rightly made, correspond strictly with all he had previously claimed.

He adds a practical bit of information as to the progress of the improvements he was making, viz: that he was using his wide-angled glasses with glycerine as the immersion medium, the density of the medium thus operating practically as if the cover-glass had been thickened. (*M. M. J.*, xiii., 23.)

Microscopists of this country know that this medium, of refractive index 1.475, was in use with us from the time Tolles introduced it with the distinctly stated purpose of approximating the index of the glass front ; but I have not noticed any evidence of appreciation of it on the other side the ocean, till the more recent renewal of the interest in homogeneous immersion glasses. Mr. Wenham, curiously enough, had sneered at "playing with extra thick cover-glasses," wholly missing the point that by building up the cover-glass till it filled the space between objective front and object, you were getting glass itself (instead of a medium of the same index) between them.

Tolles taught that the collar adjustment must be varied for change in length of tube and in the obliquity of the illuminating pencil of light, and consequently always insisted that a first-class objective should have the cover-adjustment even if used with balsam as an immersion. American microscopists have uniformly followed him in this, and those of Europe are rapidly coming to the

same conclusion, so that the question of the medium is no longer one of dispensing with the screw collar, but is practically limited to the extra apertures that may be obtained by using an immersion fluid $2\frac{1}{2}°$ greater index than glycerine. I do not know that any glass has yet been made of a greater angle than would be available with glycerine immersion alone. Charles A. Spencer told me, a short time before his death, that the capacity of glasses corrected for glycerine immersion was yet far short of being exhausted. The proof is a simple one. The same formula which gives us 82° in the glass front as the limit of a dry glass, and 122° as the limit of a water immersion, makes 159° the angle which is possible with glycerine.

The great discussion now languished for a time over the question of the method of using a slit in measuring the aperture of an objective, in regard to which Mr. Wenham, who had abandoned his tank for it, now contended that all glasses were over-estimated by all methods in use. As this applied to English and continental objectives as well as to Mr. Tolles, it called for no further argument from him in particular, and he contributed to the debate only one or two brief letters more. The battle was won.

Two devices to demonstrate the use of rays in excess of 82° in glass as "image-forming rays" are worth remembering. One was Mr. Wenham's own "reflex illuminator" which Mr. Samuel Wells of Boston used for giving bright light illumination in the actual examination of test objects, and to which I have already alluded. He described it in the Boston *Journal of Chemistry* for June, 1875, and the description was copied in the London *Monthly Microscopical Journal* for July, p. 30. As the device was invented to give black background by total reflection of all rays of light of less than the critical angle, it was a peculiarly conclusive *argumentum ad hominem* when Mr. Wenham was asked if bright light illumination with it must not be by a pencil of more than 82° in the glass.

A couple of years later Dr. Woodward modified another of Mr. Wenham's devices for the same purpose. It was the rectangular prism attached by immersion contact to the bottom of the object-slide, with an arm by which a beam of light through a pin-hole was the only illumination admitted to the prism, and this perpendicular

to its face. No aperture less than 90° in glass or balsam could, therefore, be available for the resolution of objects with it. (*M. M. J.*, xvii., 61.)

The controversy was now removed within the Royal Microscopical Society itself. In February, 1874, the President of the Society, Mr. Brooke, in his annual address had sustained the position of Mr. Wenham in regard to the measurement of the Tolles lens. (*M. M. J.*, xi., 93.) Again in the following year he unqualifiedly endorsed Mr. Wenham's measurement of aperture by the "isosceles triangle," in which, as we have seen, working distance is taken for focal distance. (*Id.*, xiii., 97.) He then, however, notices the Powell & Lealand new formula one-eighth "with an avowed single front lens," as a "decided improvement in the construction of object-glasses." This we now know was made on the principles which Tolles had been battling for, though, as President Brooke said, "its construction has (had) not been made public." With that glass, our distinguished guest and his colleague, Dr. Drysdale, made many of their brilliant discoveries in the life history of septic organisms, and, if I remember rightly, with it alone of European lenses was he then able to see the flagellum of *Bacterium termo*. (*Id.*, 106.) Influenced by his praise of the lens, I early procured one of them which has ever since been a valued part of my battery, and for some time, whilst the theme was new, took pleasure in showing that its aperture was in excess of the critical 82° in balsam. So great, however, was the influence opposing the theory of wide-angle glasses, that the makers of the objective did not publish its angle, and it still figures in their catalogue of that time as having an angle of only 140°. Of course Mr. Powell knew exactly the aperture of every glass which went through his hands, and business prudence is the only motive which could have dictated this course.

The attention of the Royal Microscopical Society had now been challenged in a way that set all appeals to mere authority at defiance. Mr. Mayall and Mr. Crisp exposed the errors in measuring the aperture of Tolles objective, and procured the endorsement of a distinguished English physicist, Prof. Stokes, for the theory of the wide-angled glasses and the computation of Prof. Keith. The authority of Prof. Abbe was already upon the same side, and his

later series of lucid articles systematized the instruction of microscopists upon the subject. Mr. Wenham has, so far as I know, never retracted his errors, nor apologized to Tolles, but every point for which the latter contended has passed into the common possession of the scientific world, as the indisputable scientific truth upon the subjects he was treating. He was right, theoretically and practically, in every assertion he made, and he knew he was right. He understood (to use again Coleridge's phrase) the ignorance of his opponent, whilst the latter was also ignorant of his (Tolles') understanding, yet he never, in a single instance, allowed himself to say a discourteous or irritating word. From the very first article of the series he was jeered at and "chaffed" as ignorant of the primary principles of optics and not understanding the simplest laws of refraction, but his answers were as impersonal and as free from all indication of feeling as if dictated by the pure Spirit of Science itself. It was not simply the patience of one who is convinced that "he laughs best who laughs last"—he did not laugh at all. He saw his opponent shift to his own ground and assume the truth of what he had denied, or forced to deny the truth of what he had before affirmed, but he was content with the result, and did not even once shout "Victory." It was for him enough (would that it were so for all scientific disputants), absolutely enough, to demonstrate the error to which he was opposed.

I wish to say, without limitation, that I do not remember anywhere to have met with an earnest dispute, extending over years as this did, in which so perfect a model is furnished of the spirit which ought to rule a scientific debate; and it would be hard to find one in which greater provocation was given. We have no right to forget that Tolles' calm equanimity, accompanied by intrepid assertion of his conclusions, was a mark of high intellectual ability and large grasp. He made no pretence of liberal education, but he made no appeals to lenient judgment of his arguments on that account. With simple self-respect he stated the truths he saw clearly. It is *our* business to recognize and record the fact that all this revealed the clearest evidence of good education. Science will not enquire whether he got it in the schools, or, as Spinoza did, in the intervals of his labor in making lenses. The verdict must be that he had it, and

that it was fully adequate to a controversy in which, for years, he practically had to contend with the organized authority of the Royal Microscopical Society.

He was not simply a practical "rule of thumb" man, who made improvements he could not explain, and who was indebted to others for an intelligible explanation of them. His own ideas were the clearest of all, and despite a somewhat rugged and over-terse style of speech, his own arguments were the strongest as well as the earliest that were put forth in the controversy on the side of the truth. His conception of the capability of immersion glasses was, from the first, absolutely clear, and he embodied it in practice and worked toward its perfection with strictest application of correct principle. His devices for demonstrating each controverted point in turn, were as happy and ingenious as is possible, and excite the same admiration we feel for the happy adaptation of experiment which makes the charm of the lectures of great teachers like Tyndall, and which is no mere trick of mechanical skill, but the legitimate offspring of perfect lucidity in the application of theory to practice. He seized upon the conditions his opponent presented, and turned his experiments and the consequent argument against him with an easy mastery which only judicial blindness could have prevented that opponent himself from seeing.

Even his friends conceded, however, that his style was uncouth and his articles ill expressed; but I make the confession, for one, with hearty shame, that what I joined at the time in thinking so, was only a terseness which assumed a knowledge that was at least readier than his readers actually had. It was as a mathematician will use a formula, to him familiar, but which we do not readily supply the proof of. Reading those articles again in the preparation for this paper, I confess that the obscurity was in the reader and not in the writer. They were too earnest in pressing directly for the mark, to be elegant; but whoever will read them in the light of knowledge of the subject we have to-day, will see that the literal truth is that their only difficulty grows out of an elliptical style which omitted the simpler steps of a proof which, with his clear apprehension of it, he thought we could supply for ourselves.

There is nothing muddy about them; nothing confused or inac-

curate; they are simply abbreviated and condensed so as to make them hard reading to any who had not a complete comprehension of the subject. And then, with the loud outcry there was from those high in authority that it was all pitiable gibberish, "beyond the pale of argument," it was much more flattering to our self-esteem to agree with them, than to humble ourselves to the work of learning what he would teach us.

The re-perusal of the controversy has impressed me deeply with the conviction that we have lost not merely an artisan of skillful eye and wonderfully delicate touch and skill, but a man who had made himself in the largest sense master of the science which lay beneath his art, and held its principles with an easy strength of grasp which is rarely matched. One who was by common consent ranked among the leading experts in geometric optics, and whose mere authority ruled for years a great scientific association, was worsted in the debate, not because he was not worthy of his reputation, but because the greater mastery of the principles of optics made it easy for Tolles to put the exposure of his errors in a light almost ludicrous from its completeness.

It is matter for satisfaction that this Society made him an honorary member of it during his life, not merely because he had done much for microscopy by the improvement of our object-glasses, but because he had given such evidence of scientific attainments as to put his name in the honorable list of men who like Lister and Andrew Ross were inventors because they were scientists, and worthy to rank with the investigators who made use of their lenses in the various departments of scientific inquiry. I am glad also to perform the duty of placing on our records a summary of Tolles' part in the controversy which has been narrated, that it may encourage others by showing that the Society will not willingly allow worthy achievements in any department of miscroscopical work to be forgotten, but will endeavor to keep them in perpetual remembrance. We have young American opticians worthy to be successors of Spencer and of Tolles, and they may be assured that zeal in mastering everything which science can teach and art can apply in the further development and perfection of the microscope, will not be overlooked or forgotten; but our successors will take pride in record-

ing it as we now do in recording our judgment that Robert B. Tolles is entitled to a permanent place in the list of the world's scientific worthies. May they rest content with nothing short of being as "easily chief" in their department of work as was he!